

SINGULAR SPECIMENS

OF THE

EDINBURGH PRACTICE

OF

CRITICISM.

BY

JOHN JOSEPH GRIFFIN.

"This discourse scarcely presumes to speak of criticism, as it now lives and flourishes. Much, however, of the pleasure of literature arises out of its skilful exercise. If there be in it little of the splenetic heart of a former century, *there is abundance of untimely fruit, and confident foreheads*. Its defects are twofold,—A WANT OF MODESTY, AND A WANT OF KNOWLEDGE. A remedy for the former is to be found in the removal of the latter. The truest critic, like the deepest philosopher, will produce his opinions as doubts. Only the astrologer and the empiric never fail."—*Pleasures, Objects, and Advantages of Literature*, by the Rev. R. A. WILLMOTT.

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A LETTER

TO

JOHN HUTTON BALFOUR, M.D., F.L.S., F.R.S.E.,

PROFESSOR OF MEDICINE AND BOTANY IN THE UNIVERSITY OF EDINBURGH.

LONDON, *June 7, 1851.*

SIR,—Some time ago I offered you a sum of money to write a book for me. You accepted the offer; you wrote the book; you received the money; you conveyed to me the copyright. The work was entitled, “A MANUAL OF BOTANY, by PROFESSOR BALFOUR.” When you agreed to write the work, you pledged yourself to me *to use it for your TEXT-BOOK, and to do all you could to promote its sale.* Very lately, however, an anonymous libel on the work, evidently written with the desire to *destroy its sale*, has appeared in a Scotch newspaper, the botanical department of which is said to be under your guidance, and in which it is admitted by the publisher that you write the leading articles. That libel has been printed as a hand-bill, and extensively circulated. I have brought the subject privately under your notice, and received no reply nor redress. You have not disowned the article, nor declared your disapproval of it, but, on the contrary, while the libel, damning the Manual of Botany, is allowed to take its course, and has indeed been forced through every possible channel, Messrs. A. & C. Black of Edinburgh, are advertising a work called “BALFOUR’S CLASS-BOOK OF BOTANY,” a work which, I presume, is intended to displace the Manual that you wrote for me, for money, under the pledge to use it as your Class-Book.

These circumstances force me to address you thus publicly.

The libel of which I complain is given in the Appendix, No. 1. I have added my letter to you on the subject, (No. 2,) and the reply to that letter received from your publisher, (No. 3.)

I inquired of you whether *you had authorized* the publication of the article. Your publisher replies that *you are not responsible* for it. But as you have taken no pains, either privately, or publicly, to disavow it, since it was formally brought under your notice, I consider myself justified in concluding, *that you did authorize* it, and *that you are responsible* for it, and I shall continue to hold these opinions till you disprove what I am about to state in this letter.

I subjoin a letter (No. 4) received from the editor of a London journal, who, having written to you, asking for a copy of the Second Edition of your Manual of Botany, for the purpose of reviewing it, received, *as the only reply*, a copy of the printed hand-bill containing the libellous review from the *North British Agriculturist*. “Unable to make use of *SUCH a slip, without even authority*,” the editor wrote to me to explain it. I sent him a copy of the Manual, with the request that he would examine the work, and judge for himself; and I added, that Dr. Balfour had indeed not edited the second edition, in consequence of a dispute that had arisen between him and the publishers, on money matters, but that the publishers possessed the Copyright of the work, and had been authorized to publish it, and that the charges made in the review, that they had destroyed the book by blunders and bad editing, were quite unfounded, as an examination of the work would prove.

This letter received the reply contained in the Appendix, Nos. 5 and 6. Professor Henfrey’s resolution “*to do nothing to advance the sale of the work in question*,” will no doubt gratify you, as will also the perusal of the documents Nos. 7 and 8, which show, that the book has been excluded from the Universities of Glasgow and Cambridge.

There can be no doubt that this simultaneous condemnation of the book is due entirely to the industry with which the hand-bill containing the “Review,” No. 1, has been circulated wherever the ingenious gentleman who sends them considered it possible to do mischief, among your friends and mine—among Booksellers, Professors, Reviewers, and the like. Several copies of the hand-bill have been sent to me by their amazed receivers.

I admit, if it is any gratification to you to have the admission, that this Review has already done the Manual a great deal of harm, and that it is able to do a great deal more. I have applied to you to prevent it. You have declined to do so, and I am forced to publish the following particulars in self-justification.

That you are preparing a New Class-Book is admitted by you. That you are doing so, as alleged by Professor Henfrey, in “SELF-DEFENCE” against an “UNAUTHORIZED” publication, must be judged of in connection with the facts that follow. Where there is *no attack*, there needs little *self-defence*.

In the year 1847, I formed the idea of publishing a series of *Cheap Manuals on the Natural Sciences*, and in looking about for literary assistance, I applied to you. In the Appendix, Nos. 9 and 10, I give my letter and your reply.

These letters prove that the *idea of the Manual* ORIGINATED WITH ME, and that, although an undefined vision of a *Cheap Text-Book*, had floated before your eye, you did nothing towards realizing the object, until I discovered the means of overcoming the “*great difficulty*” in the way of *establishing* a Cheap Text-Book, namely, till I *produced the necessary MONEY*.

In February, 1847, I resolved to visit London and Paris, to make arrangements for carrying out my scheme, and to pass through Edinburgh on the way up, to ascertain what literary aid I could obtain there. On the 11th February, I called upon you, and it was agreed between us that you should execute the volume on Botany, *provided*, that upon my arrival in Paris I should still think

it expedient to purchase, and should succeed in obtaining, the cuts of Jussieu's Manual. The knowledge, possessed by both of us, that the French publisher had *hawked* some of his casts too much in England, rendered it expedient that I should make certain inquiries before buying them at all. It was agreed, that if I bought them I was to let you know immediately, that you might proceed with the work, and have it ready for use the next season. But, whether you should, or should not, write the work, was left entirely contingent upon the fact, whether I did, or did not, purchase the casts of the cuts of Jussieu's Manual. It was not the case that you were, *at all events*, writing a Text-Book, of which I was to have a license to print an edition; but it was the case, that I was projecting a *series* of publications, on one of which you were to work, provided that, on my arrival in Paris, I should still think it expedient to carry the project into execution. If I had failed in obtaining the engravings, your Manual certainly would not have been written for me, and, perhaps, would never have been written at all. To such an extent was the enterprise *mine*—not *yours*.

On arriving in London, I found that the Encyclopædia Metropolitana was for sale, and perceiving in that work the means of giving great support to my plan of publishing a cheap series of scientific manuals, I purchased it for the Company with which I am connected, and with the intention to extend and complete it, in accordance with Coleridge's original plan. This purchase prevented my journey to Paris. Nevertheless, on the 7th April, I wrote you word that I had purchased the casts of the botanical cuts, and I urged you to proceed with the Manual rapidly. It was to have been ready by the spring of 1848, but you did not complete it till the spring of 1849.

On the 10th March, 1849, you wrote to me as follows:—

"I presume that I may now draw upon you for £200, agreed upon for the present edition,—say at 30 days."

When I planned the work, provided the cuts, and offered you £200 to translate Jussieu's treatise, it was, of course, in order that I might acquire the copyright. I never agreed to pay £200 for leave to print *an edition* of your Text-Book. *You had no text-book* at the time, and it was not stipulated whether I should print 1000, 2000, or 20,000.

Out of much correspondence that passed between us, I shall quote two letters to show my view of the matters in dispute. Appendix, Nos. 11 and 12.

After a tedious correspondence, during which solicitors were consulted on both sides, and which confirmed me in the resolution not to pay you the £200, until you assigned to me the copyright of the work, you finally, on the 9th April, 1850, received the money, and gave me the following Receipt and Assignment of Copyright:—

EDINBURGH, 9th April, 1850.

£200 : 0 : 0.

Received of John Joseph Griffin and Charles Griffin, Publishers in London, the sum of Two Hundred Pounds Sterling, on the Terms that I assign

to them, as I hereby do, my interest in the Copyright of a work entitled, "A Manual of Botany," founded on the model of *De Jussieu's Cours élémentaire de Botanique*, and consisting in part of a translation of that work undertaken at their request; and I authorize them to publish the said work, on their own account, in any manner they think fit, and to enter themselves in the Registry Book of the Stationers' Company of London, as the proprietors of the said Copyright.

(Signed)

J. H. BALFOUR.

(Signed) I. BAYLEY, of the City of Edinburgh, Solicitor,
Witness.

WM. GAYLOR, of the City of Edinburgh, clerk
to the above Isaac Bayley, Witness.

I charitably assume, that when, as your Reviewer states, you recently informed your class that "*you had not sanctioned the publication of the second edition of your Manual of Botany*," your treacherous memory had forgotten that this assignment contained the following clause:—"I authorize them to publish the said work on their own account in any manner they think fit."

In June, 1850, it was necessary to make arrangements for printing a second edition of the Manual of Botany. Accordingly, I wrote to you on the 18th June, inquiring *whether it would be agreeable to you to revise the work, and on what terms?* Your reply is given in the Appendix, No. 13.

I again wrote to you on the 27th June. Appendix, No. 14. This letter received no reply. On the 9th October, 1850, I wrote again to you on the same subject, and received no reply.

I shall now cite a few, out of many, offers that have been made, to induce you to revise the Second Edition of the Manual of Botany.

I. Proposals to ensure to you, at all times, complete Literary and Scientific control over your Manual.

1849, *March 28th*.—"You shall retain the right to make corrections on the work every time it is reprinted, provided you complete the corrections within one year from the time when notice is given to you that a new edition is required. You agree to bring up every edition of the Text-Book to the state of science at the time of its publication, and to write, or use in teaching, no other Text-Book. If any dispute should arise respecting the quantity or kind of corrections required by the book, or any other thing relating to it, the author and publishers agree to submit the dispute to the decision of two arbitrators, one to be chosen by each party, whose decision shall be binding upon both parties."

1849, *April 3d*.—In your reply to these proposals you objected:—"You would chain me down to use no other work for my class, whatever the price of the Manual might be, (and I see you have already added to the price originally contemplated)." The addition to the price of the work was rendered necessary by your extension of it beyond the agreed limits, and by the expenses you caused upon it; nevertheless,—

1849, *April 5th*.—I met this objection, by agreeing that the arbitrators to whom I proposed to refer matters of difficulty, should have the power to *fix the price of any edition*, after hearing your arguments respecting it, and mine. I also agreed, that they should have power to permit you to use any body else's Text-Book, instead of your own, if they thought it reasonable, and you thought so too.

In that same letter, I offered to go to Glasgow and meet you before *two neutral uninterested respectable men*, and to empower them to decide the dispute between us, in my favour or in yours.

You refused to meet me, or to make any arrangement, except on the previous understanding, that the copyright should be yours, which, of course, I declined.

II. *Offers of Money made to you.*

These have been so numerous that it would be tiresome to quote them all. I shall select two examples, which I may preface with the remark, that you have repeatedly declared that pecuniary recompense was a minor consideration with you.

Proposal of April 6th, 1850.—a) To pay you £50 for editing a second edition when required. b) To pay you for editing a third edition, one shilling per volume on all that were printed. c) At the end of three years after the publication of the third edition, to assign to you the entire copyright, *gratis*.

Proposal of November 15th, 1850.—To pay you £100 to correct the Second Edition, without reference to payments for any other edition.

You refused to accept these offers.

I quote them, that men of business may judge of their fairness. Every reasonable inducement was held out to get you to edit the Second Edition. I did not think that the work required much to be done to it; but what it did require, I wished you to do. I guaranteed to you unlimited power over the scientific and literary management of your Manual; and I consider that the offers of money made to you were as liberal as a man in your circumstances had any right to expect, the original outlay upon the work not having yet been repaid by some hundred pounds.

Nothing, however, could please you but the entire possession of the work, and you appeared to be insensible of the extravagance of your proposal. Imagine a railway company to employ an engineer to construct a railway. When it is done, he takes payment of his account as engineer, and claims the railway as his property, because he made it. The capital and labour of the company go with him for nothing. Just so it is with you. The Manual was invented by me. I found the money to pay for the casts, and for all the other engravings that were cut; for the printing of your nearly-illegible MS.; for the books that were made imperfect by the forced sale of odd sheets among your pupils; for nearly 50 copies of the Manual given away to your friends, professors, and reviewers, in order to show the book; for a large amount of advertising; for payment of the copyright. I may add to this, the effects of my own exertions and those of my partner, and the power of an established business exerted to make

the book succeed. When success is attained, you come forward to claim as *yours* the property produced by this combination of capital and labour. Nothing so perfectly extravagant is to be found in Mr. D'Israeli's *Curiosities of Literature*.

The reader will now understand our respective positions just before the Second Edition of the *Manual of Botany* was put to press. I was still desirous of agreeing with you, willing to pay you what I conceived to be your due, but determined not to surrender, at your dictation, property to which you had no title. It was necessary to put the work immediately to press, and as the first edition had only been finished eighteen months, it occurred to me that the work could require but little alteration. Though botany is a progressive science, it does not, as your Reviewer pretends, become antiquated in eighteen months. Moreover, as I believed your resolution to repudiate the work could not endure long, when you saw that you were unable to coerce me, I resolved to reprint the work *verbatim*, correcting only the evident errors of the press, and relying upon your recovering a more reasonable state of mind by the time a *third* edition was required. I was merciful enough to believe, that you would not entirely abandon the line of conduct that men of business are accustomed to follow. With this view, I employed a gentleman well acquainted with botanical literature, to edit the Second Edition of the *Manual* anonymously. I gave him instructions to the above effect, and he fulfilled his duties, as I think, honestly and creditably.

The Second Edition of the *Manual of Botany*, thus prepared, was duly published, and shortly afterwards the "Review" to which I have referred, appeared in a newspaper with which you are connected so intimately, that I cannot be far wrong in presuming that you *saw the review before it was published, and knew that it was to be published, and did not prevent nor forbid it*.

Whether this is the case or not, if the Review were a fair and honest critique, written to serve the cause of science, I should have no right to complain of it. Neither should I have any right to complain of the condemnation of the book, if it were true, that by bad editing, the work had been put into a condition that was injurious to your reputation.

My complaint is, that the charges made in the Review, and the colouring given to them, are, for the most part, false, and that you, nevertheless, permitted the article to be printed as a *bona fide* Review; and when it was formally brought under your notice, after publication, did not disavow nor discountenance it.

Upon the publication of the Review, I sent it to the Editor of the Second Edition of the *Manual*, whose reply to it I give in the Appendix, No. 15.

After reading the attack and the Editor's reply, I have compared the First Edition of the *Manual* with the Second Edition, prepared for press by him.

The Review states that "there are imperfections in this edition of a more serious nature" [than those in the first edition];—that the "former errors are multiplied, so as utterly to destroy all confidence in the book as a work of reference."

In opposition to these reckless statements, I declare that I have counted the

errors existing in the First Edition, and corrected in the Second Edition by the Editor, and that they amount to FOUR HUNDRED AND NINE. The marked copy was fortunately preserved. I have numbered the errors, and will show the volume to any body who can produce a list of four hundred and *ten* errors in the Second Edition, to justify your reviewer—though 410 is not exactly a *multiple* of 409, and the statement is, that the errors are *multiplied*.

With regard to the quality of the errors thus corrected, I may class them as follows:—*a*) Bad English; *b*) Mistakes in proper names; *c*) Errors in Latin; *d*) Errors in Greek; *e*) Errors in Hebrew; *f*) Errors in matters of fact. The list is too long for quotation; but I shall give a few examples in Appendix, No. 16.

The special attack on the Hebrew names leads me to say a few words on that subject. The total number of Hebrew words in the volume is 29, of which 13 were incorrect in the First Edition, and only 9 are incorrect in the Second Edition. Of these 9, several are incorrect only because the editor, overrating your scholarship, took for granted that your Hebrew names were correct, in which, unfortunately, he was deceived. *There are only two words incorrect in the Second Edition that were not incorrect in the First Edition.* There are six that were incorrect in the First Edition, that are correct in the Second Edition. There are seven incorrect in both editions. There were sixteen correct words in the First Edition, and there are twenty correct words in the Second Edition. These facts contrast strangely with the statement in the Review, that the Second Edition “has BECOME a *false guide* to the student of Scripture plants.” It required considerable audacity to set forth, that 16 correct, and 13 incorrect, Hebrew words, contained in the first edition, constituted “a *true guide* to the student of Scripture plants.”

It is scarcely necessary to add a word to the Editor’s reply to the extravagant condemnation of his transposition of the order Rhizanthææ. The Review admits that “there has been much dispute concerning it.” You admit yourself, that “its place in the natural system is still doubtful,” and that “Lindley has placed it in a separate class, intermediate between Thallogens and Endogens.” (*Manual, First Edition*, p. 429.) This is exactly where the Editor has placed it, for which, moreover, he adduces the authority of Endlicher. It is surprising that you should have permitted your critic to speak of this matter in the bombastic terms employed in the review:—

“Verily, this is worthy of *Punch*; a powerful satire upon systems of classification; a perfect outrage upon common sense. A palpable error like this could only *occur in the hands* of one profoundly ignorant of the merest rudiments of Botany, and it stamps the edition as perfectly disgraceful to a scientific Botanist.”

This delectable vituperation is poured upon the Editor because he thinks Lindley and Endlicher better authorities than Balfour, in reference to the disposal of one debateable *class* or *order* (for even that point seems to be still undecided) of plants.

The objection made to my putting the work into the Encyclopædia Metropolitana is remarkable. It would not, however, deserve notice, but that I per-

ceive it has acted on the mind of Professor Henfrey, (Appendix, No. 6,) who refers to it as to a grievance. I put your work, instead of Mr. Don's, into the Encyclopædia, because it was a newer, and, as I thought, a better work. I suppressed Mr. Don's article that it might not compete with yours. How does that aggrieve you? Both copyrights being mine, I used the work that I liked best. There is, however, one point on which I can comprehend your difficulty. It is, that the man who can make so many strange mistakes in Greek, Latin, Hebrew, and English, as exist in your First Edition, may well object to have his work brought into close proximity with those of the chief scholars of Oxford and Cambridge, some of whom might be a little diverted to be told by an Edinburgh Professor, that ἡὼς means *drawn*; that ἑλῖξ means a *ring*; and that *fero* and *φέρω* mean to *bear*.

If the Second Edition of the Manual *had been read* before it was *reviewed*,* it could have been discovered, that 409 errors had been corrected; that the Hebrew, though still incorrect, was more correct than in the First Edition; that the Greek, Latin, and English, were freed from hundreds of errors, and that the work was, in other respects, precisely the same as the first edition. But the Review states,—1) that the imperfections remain uncorrected; 2) that there are imperfections of a more serious nature [than those in the First Edition]; 3) that the errors are multiplied; 4) that there is nothing new in this edition deserving of the slightest commendation; 5) that in all instances of alteration and attempted emendation, the severest censure is due for the utter want of judgment and knowledge of the science; 6) that certain changes of a sufficiently odd and remarkable character have been introduced, which utterly destroy all confidence in the book as a work of reference; 7) that the work has been rendered ridiculous by bad editing; 8) that Professor Balfour's writings are distorted, and his name is associated with a volume of blunders which have been brought together by other hands, &c. &c. &c.

It is unnecessary for me to *characterise* a "Review" got up on this plan.

I must add a few words respecting the MICROSCOPES that were, at my desire, described and figured in the Appendix to the Second Edition. Your Review states, that "these are certainly not the kinds of microscopes which *our* histologists of the present day [the language is felicitous!] would recommend more especially, one having access to the excellent instruments made by Powell, Ross, and Smith, in London, and by Oberhauser and other first-rate opticians in Paris."

I am far from undervaluing the microscopes made by such men as Mr. Ross, any more than I undervalue the telescope made by Lord Rosse. "*Our Astronomers of the present day*" are perhaps right in recommending Lord Rosse's telescope; and I believe that Mr. Ross's microscopes deserve their high character.

* That eminent Edinburgh Reviewer, the Rev. Sydney Smith, is said to have advised somebody never to read a book before he reviewed it, lest he should be prejudiced. I presume that, upon such excellent authority, this principle is followed by the critics of the *North British Agriculturist*, whose morals seem to be derived from Deau Swift's Advice to Servants.

But, I ask, do you, as a teacher of medicine and botany, recommend your pupils to buy microscopes of Ross, at a cost of £40, £60, or £80, and to use none that are cheaper? If you give that recommendation, are you obeyed? I can understand that you may occasionally have a rich pupil, and one who may be anxious to fulfil your wishes in every particular, and in that case your recommendation may be implicitly followed. But the recommendation of such microscopes to medical students is absurd, and the sneer at cheap microscopes is unscientific. Precisely the same objection was made to cheap chemical apparatus when I first introduced it, fifteen years ago. The sneerers had soon to change their tone in relation to cheap chemical apparatus, as you will have to do in relation to cheap microscopes. Your students can never have *any* microscopes till they become cheap, and, therefore, the sneer at an attempt to accomplish so desirable an end, as the production of a cheap microscope, is little to the credit of a man of science.

The aspersed microscopes, though not to be compared with instruments made by Mr. Ross, and sold at six or eight times their price, will afford that aid to the botanist and medical man, that no other microscopes have hitherto done at less than three times their price.

CONCLUSIONS.

I commit these details to the press with much regret. But the documents show that it is necessary for me to print them. It is right for you, as a man of science, to protect your reputation and your property. It is right for me, as a man of business, to do the same. You and I have quarrelled, as many have done, on money matters. I offered at one time to submit the point in dispute between us to private arbitration; that failing, I expected to meet you in a Court of Law. When I demanded the Copyright of the work for the £200, and refused your offer of a license to print an edition for that sum, you might have sued me for the money, and tried to establish your case in a Court of Justice. You preferred to take the money, and assign to me the Copyright;—and I commend your discretion. Having paid you for this property, I considered it to be mine, and that I had a right to use it. But now I find myself defamed, and my property injured, by a Review, written, it is said, to sympathize with you, and to advise you to take care of your reputation, and published in a Newspaper with which you are very intimately connected. This review is copied into a hand-bill, and circulated most industriously wherever it seemed likely to be able to do me any mischief. The effect is, that the defamed book is excluded from the Schools, and I am stigmatised, as publishing, WITHOUT AUTHORITY, an edition of your work, so incorrect as to damage your reputation—the TRUTH being, as I have shown, and as you cannot disprove, in direct opposition to the statements contained in this so-called Review.

I appealed to you, who know the facts, to protect me against these falsehoods, and to support me in the possession of the property for which I paid you; and the only reply to that appeal is, an advertisement, by Messrs. A. & C. Black, of “PROFESSOR BALFOUR’S CLASS-BOOK OF BOTANY.”

Your letters, in my possession, contain abundant evidence that you pledged yourself to me to use the Manual as *your Text-Book*. Trusting that you would do so, I paid you a large sum for the Copyright—much more than I would have done had I not trusted to your promises and assertions. You have, in my opinion, no right either to publish a New Text-Book, or to suffer the defamatory Review in the *North British Agriculturist*, to remain uncontradicted. I have waited a month, to give you time to contradict it, but having waited in vain, I now submit these particulars for the information and judgment of the public.

The Review says, that “the person who holds the distinguished position of Professor of Botany in the University of Edinburgh, must afford to the public some explanation of such a state of things.” Much as I despise the general tenor of the Review, this sentence receives my approbation. I have already requested such an explanation from you privately, and now I demand it publicly. I have been charged with USING and DESTROYING your writings WITHOUT AUTHORITY. I ask you either to PROVE these charges or to DISPROVE them.

I am, SIR,

Your humble servant,

JOHN JOSEPH GRIFFIN.

APPENDIX.

No. 1.

Extract from the "NORTH BRITISH AGRICULTURIST" of Wednesday, May 7, 1851.

REVIEW.

A MANUAL OF BOTANY: being an Introduction to the study of the Structure, Physiology, and Classification of Plants. By JOHN HUTTON BALFOUR, M.D., F.L.S., F.R.S.E., Professor of Medicine and Botany in the University of Edinburgh, 2d Edition. Forming Volume XIII. of the Cabinet Edition of the Encyclopædia Metropolitana. London: Griffin & Co., 1851.

The name of BALFOUR is one eminently distinguished in the science of Botany, and at once stamps a work like the present with a very high degree of authority, and entitles it to the entire confidence of the scientific reader. When the first Edition of the "Manual" was published (as a class book, not with the view of forming the article "Botany" in the Encyclopædia Metropolitana,) it was very favourably noticed in this Journal, and recommended as a suitable guide to all students of botany. We thought it a good introduction, and although there were many imperfections, yet we naturally looked forward to the removal of them in a future Edition. Judge then of our disappointment, when, on looking into the pages of this *new Edition*, we find in the first place, that the imperfections to which we have alluded remain uncorrected; and secondly, that the many important discoveries in physiology and other departments of the Science which have been made known since the printing of the first edition are unrecorded, as well as others of earlier date which had escaped the author's notice, and were pointed out by reviewers of the work. Not only so; there are imperfections in this edition of a more serious nature; and which banish all the confidence with which the work would otherwise have been regarded, on account of the author's well earned fame in the paths of Botanical Science. Had this volume of the Encyclopædia been merely a correct re-print of the former "manual," with all its minor faults and imperfections, it still could have been pointed to as an able and comprehensive exposition of the science of Botany brought up to within a short period of its publication. Instead of this, however, the errors are multiplied; and certain *changes* of a sufficiently odd and remarkable character have been introduced, which utterly destroy all confidence in the book as a work of reference to the scientific student, as well as the general reader. What is the meaning of all this? is a question which will naturally occur to the mind of every one; and we must confess that in such a state of things, some explanation would seem to be called for on the part of one who holds the distinguished position of Professor of Botany in the University of Edinburgh. An explanation has been given. The Professor has informed his class that *the edition has not been edited nor corrected by him; that he is not responsible for it; and that in fact, he has not sanctioned its publication.* We are glad for the Professor's sake, that he is thus disconnected with the new book which bears his name.

There is nothing new in this edition (so far as we can see) deserving of the slightest commendation; in all instances of alteration and attempted emendation the severest censure is due for the utter want of judgment and knowledge of the science. It would therefore be an irksome and an exceedingly unprofitable task to lead our readers through the volume; but the observations which we have offered render it expedient that we should point out a few of the scientific inaccuracies that characterise the edition. This will likewise show the amount of confidence to which it is entitled as a work of authority. In Fossil Botany the recent important researches of Brongniart are not even alluded to, and the number of Fossil plants is stated far below what Unger has described. In Botanical Geography, the erroneous statements pointed out by Hooker and Watson have been left unaltered. None of the valuable remarks in Mr. Watson's *Cybele Britannica* have been attended to, and thus he is represented as advocating views which he has abandoned. In the arrangement and description

of the characters and properties of the Natural Orders no improvement has taken place. On the contrary some glaring errors have been introduced. Thus, for instance, the Hebrew names are in many cases quite wrong, the editor not having been able to distinguish between the letters *He* and *Heth*, *Vau*, *Resh*, and *Daleth*, *Nun* and *Gimel*, &c. To the student, therefore, of Scripture plants the work has become a false guide. In the disposition of Natural Orders, one *extraordinary* case may be noted. The Natural Order Rhizanthæ, concerning which there has been much dispute, seems to have been fixed upon by the Editor as a convenient one for exhibiting new views of classification, and he settles all disputes by wedging it into the subclass *Glumaceæ*! Verily this is worthy of *Punch*; a powerful satire upon systems of classification; a perfect outrage upon common sense. Who could have fancied that the remarkable *Rafflesia* and other allied plants had *any* affinity with grasses? A palpable error like this could only occur in the hands of one profoundly ignorant of the merest rudiments of Botany, and it stamps the edition as perfectly disgraceful to a scientific Botanist. We regret, for Professor Balfour's sake, that his name is associated with such a botanical blunder. We trust that he will take steps to make widely known that *he* is not responsible for the so-called second edition, otherwise his high character as a teacher of Botany will certainly be deservedly challenged.

In the Physiological Department there is no notice taken of the physiology of cells, as developed so ably by Schleiden; nor of the recent views of Embryology, and more especially those relative to the reproduction of Ferns and other Cryptogamic plants. The Chemistry of vegetation contains only the old tables of analyses, and there is no mention of the late agricultural investigations by Anderson and Johnston, nor of Dr. Wilson's discovery of Fluorine in plants; of Mr. Way's researches as to the absorbing powers of clay, &c. In this respect the physiological and agricultural student will be disappointed.

In that part of the work treating of the microscope, two wretched woodcuts are introduced, to show two kinds of microscopes, manufactured as appears by a note from the "Editor," by Messrs Griffin. These are certainly not the kinds of microscope which our histologists of the present day would recommend, more especially one having access to the excellent instruments made by Powell, Ross, and Smith in London, and by Oberhauser and other first-rate opticians in Paris. The various *jim cracks*, for we can call them nothing else, introduced into the woodcuts of the microscopes are more worthy of a shop bill than of a work on science. The "microscopes" themselves might do for the amusement of little boys not old enough to go to school.

We dismiss the work then, with a serious regret that what in qualified hands might have been a valuable work, has been spoiled and rendered ridiculous by bad editing. The edition is got up in a style similar to the last, and the stereotypes of the French woodcuts, which were never good, have become defaced and imperfect. We observe in one case the woodcut completely inverted, and the letter indicating a particular organ placed opposite another part of the flower altogether. We sympathise deeply for Professor Balfour in having his writings distorted in this manner, and his name associated with a volume of blunders which have been brought together by other hands.

No. 2.

LONDON, 10th May, 1851.

PROFESSOR BALFOUR, Edinburgh.

SIR,

My attention has been directed to an article that appeared in the *North British Agriculturist*, of Wednesday, May 7, in the form of a review of the Second Edition of your Manual of Botany.

I am informed that you are the botanical editor of that newspaper, and that consequently the article in question must have been inserted with your knowledge and concurrence.

I am unwilling to credit this on insufficient evidence, and I am led by a sense of justice and fair dealing, to inquire directly of yourself,—*Whether you did authorize the publication of that Review?*

In the event of your not replying to this inquiry, I shall consider that you admit the statement to be true, and that I am at liberty to publish this letter.

I am, SIR,

Your obedient Servant,

JOHN J. GRIFFIN.

No. 3.

EDINBURGH, 13 HANOVER-STREET, 16th May, 1851.

SIR,

Professor Balfour has handed me your note of the 10th, I beg to inform you that Professor B., though contributing the leading articles of the *Journal of Horticulture*, is not responsible for the contents of the paper, and that the article in question was not written by him.

I am, your most obedient Servant,

DAVID GUTHRIE,
Proprietor, N. B. Agriculturist.

J. J. GRIFFIN, Publisher,
Baker-Street, Portman Square, London.

No. 4.

16 A. ALPHA ROAD, ST. JOHN'S WOOD, 15th May, 1851.

SIR,

I applied to Professor Balfour for a copy of his *Manual of Botany*, in order to notice it in the *Gardener's and Farmer's Journal*. Some one has sent me the enclosed. I shall be happy to notice this work, but I cannot make use of such a slip as this, without even authority.

I am, GENTLEMEN, yours, &c.

ROBERT MARNOCK,
*Superintendent of the Royal Botanic Garden, Regent's Park, and
Editor of the Gardener's and Farmer's Journal.*

No. 5.

SIR,

I applied to Professor Henfrey, who attends to this department, to give me a critical notice of your work, and the enclosed is the answer. For the present I must, therefore, return the vol., and abstain from taking any part in the disputed point.

Would it not be better to give a public explanation, this would enable persons like myself to judge for themselves?

I am, SIR, yours truly,

MESSRS. GRIFFIN & Co.

ROBERT MARNOCK.

No. 6.

17 MANCHESTER-STREET, GRAY'S INN ROAD, May 28, 1851.

DEAR SIR,

I thought to have seen you at the Gardens on Saturday, and intended to tell you that the so-called volume of the "*Encyclopædia Metropolitana*" is a reprint, UNAUTHORIZED by Dr. Balfour, of the first edition of his *Manual of Botany*, and that he is now preparing a new work in self-defence. Therefore I could do nothing to advance the sale of the work in question.

Yours, ever truly,

MR. MARNOCK.

ARTHUR HENFREY.

No. 7.

GLASGOW, May, 1851.

Dr. Arnott's Class all prefer the old edition of Balfour, and we feel a difficulty in selling the new edition.

Yours truly,

ALEX. HADDEN,
Bookseller, High-Street.

No. 8.

CAMBRIDGE, May 19, 1851.

DEAR SIRS,

I have sent back Six Balfour's (*Ency. Met.*) as I cannot use the book here; the men will only take the Balfour's *Botany* as it originally appeared. Will you please enclose Six for those now returned, and oblige,

Yours, &c.

MESSRS. GRIFFIN & Co.

J. DEIGHTON.

GLASGOW, *January 2, 1847.*

TO PROFESSOR BALFOUR, Edinburgh.

DEAR SIR,

We have formed the idea of publishing translations of the following three French works:—

“Cours élémentaire d’Histoire Naturelle.”

1st, Botanique, par M. A. De Jussieu.

2d, Zoologie, par M. Milne Edwards.

3d, Minéralogie et Géologie, par M. F. S. Beudant.

The cuts first drew our attention, but we are told that the matter is also of good quality. Having entered into correspondence with the proprietors of the French works, we believe that we can procure casts of the cuts. We have now to find editors.

Presuming that the volume on Botany is known to you, we take the liberty to ask, whether it would be agreeable to you to undertake its translation into English, either verbatim, or with such alterations as might be considered necessary to adapt the work for English readers. Perhaps also you can favour us with an opinion whether there is room for such a work in this country, that is to say, whether it would have many competitors of the same kind and quality?

We put this query, because we are not very well acquainted with Botanical literature. We should mention, that a part of our plan is to publish these books much below the common price of similar works. The cheap system has never yet been applied in this country to books on Natural History, but we are inclined to venture a trial, keeping up, of course, the quality of the books as high as possible.

Can you inform us where we might possibly find a translator for the volume on Zoology? Has Dr. ——— leisure or inclination for pursuits of this kind? or do you think that Professor ——— or ——— could do it?

We are, DEAR SIR, yours truly,

For RICHARD GRIFFIN & CO.
JOHN J. GRIFFIN.

No. 10.

EDINBURGH, *4th January, 1847.*

J. J. GRIFFIN, ESQ.

MY DEAR SIR,

I have long intended to bring out a *Cheap Text-Book* for Students, and your proposal seems to me the means of accomplishing this object. The work to which you allude is well known to me, and can easily be adapted to the system pursued in Britain. In accepting the proposal I shall feel myself at liberty to make some modifications. In fact, I shall take the “Cours élémentaire” as the groundwork, and upon it form my *Text-Book*.

The great difficulty is in procuring wood-cuts, and I am glad to think that you are likely to succeed in getting over it. I shall be glad to talk over matters with you, and as I purpose to be in Glasgow for a few hours on Thursday next, I shall endeavour to call upon you in the afternoon, and make the arrangements necessary.

——— would be an excellent person to take up the Zoology. I shall speak to him on the subject. If he does not consent, you should apply to ——— or ———.

I am, yours sincerely,

J. H. BALFOUR.

No. 11.

LONDON, *March 22, 1849.*

PROFESSOR BALFOUR.

DEAR SIR,

If you will take the trouble to read my letter to you of 2d January, 1847, and your reply of 4th January, and indeed the subsequent correspondence, I am sure that you will perceive that the position you have assumed in respect to the copyright of the work on Botany is quite untenable.

Whatever you may have *contemplated* regarding the copyright, you certainly never *bargained* for what you now *claim*. The project, as the correspondence shows, was entirely mine, and I applied to you to execute one portion of a large scheme. My

first question to you was, will you translate Jussieu's book, either verbatim, or with such alterations as may be necessary to adapt it for English readers? You replied, that you accepted the proposal, but would use Jussieu's book only as the groundwork of yours, and modify it to suit the system pursued in Britain.

Then when I saw you, you said, it is a question of labour, how much will you pay? I said £200. Whereupon you agreed to write the work, without stating any conditions whatever as to the reservation of copyright.

I do not believe that one single word was said by either of us respecting a second edition. The only thing talked of, so far as my memory serves me, was, what was to be put into the work, what new cuts, &c. &c. In fact, a second edition did not then require any consideration on my part; for my plan of producing a *Series of Cheap Text-Books* consisted in part of *stereotyping* them—a plan which was afterwards abandoned as respects the Botany, because it was found that the wood-cuts would not print with sufficient beauty from stereotype plates.

Jussieu's book sells for Six *francs*, and our understanding was, that the English book was to be made as nearly as possible of the same size and price. My motive for keeping down the price is stated in my first letter to you, referred to above, viz. an intention to publish for the first time in England, a series of books on Natural History, of first-class quality but low price.

The least reflection will show that I could not pay £200 for copyright, £100 for wood-cuts, and other large sums for paper, printing, and advertising, and other expenses incidental to the establishing of a work, with the slightest prospect of reimbursement out of *one edition*.

No *cheap text-book* on any science can be supplied to the public except where the publisher speculates on a sale sufficiently large to cover the original outlay. You know perfectly well, and the correspondence shows the fact clearly, that my plan was to have a *SERIES of cheap text-books*.

How could the integrity of such a series be secured if I had granted, or were to grant, to each author, the power to take from me his book whenever a sufficiently large expenditure upon it had made it a valuable part of my series? I never thought of making, or did make, so extravagant a bargain. No publisher, with the least knowledge of his business, or able to compute the expenditure upon a publication, and the probable revenue it may yield, could ever have made so absurd a contract as that you impute to me.

But it seems to me that you mix up two different questions in the matter, and it may be, after all, that we are disputing, as often happens, upon a point which is misunderstood. The question of copyright of the existing work is one thing, and that of payment to be made for labour to be spent in revising a future edition, is another. What I claim is, the right to sell as many as I can of the existing work without any other payment than the £200 for copyright. But I do not claim the right of calling upon you to do any thing further to the work, after you have once completed it, without paying you a reasonable sum for what you may have to do. I do not want you to keep the house in repair, as well as build it, for the sum agreed upon—nor is it probable that I should like to live in the house and allow it to fall about my ears for want of repairs when they become necessary.

With regard to the delay in preparing the book, I must state that your early letters contain the most distinct promises to have the book ready for publication in the spring of 1848. You know that —'s translation was offered to me and declined, and that it was then accepted by ——. Whatever may be the demerit of the book, there is no doubt that many persons have bought it who would have bought ours, had ours been first in the market. I mention this, merely because you have been, as I think, unjustly angry with me for complaining of the delay.

I am, DEAR SIR,

Yours very truly,

JOHN J. GRIFFIN.

No. 12.

LONDON, April 23d, 1849.

PROFESSOR BALFOUR.

DEAR SIR,

Your having declined my proposal to submit the matter in dispute between us to arbitration, rendered my journey to Glasgow unnecessary, so that I had no occasion to write and make any appointment with you.

In consequence of your resolution, in regard to the copyright, I have considered it proper to submit the correspondence that has passed between you and me, to my Solicitor, whose opinion I enclose. I believe that any impartial person, who reads the letters that passed between us *before* I saw you in Edinburgh, would corroborate his conclusion, that the work was written, at my instigation, to form one of a series of cheap scientific manuals, projected by me, and that the copyright of it was to become mine, in consideration of the sum of two hundred pounds agreed to be paid to you for writing or translating it. My first letter to you places this beyond a doubt. I did not ask you for a license to print an edition of your Text-Book, for you had no Text-Book in existence, but I asked you to translate, or write, for me, one of a series of Manuals which I intended to publish, and three of which were named, and you agreed to write one of them accordingly.

That the work would be used by you as a Text-Book was a contingency upon which I calculated before applying to you to write it.

The original French work, of which yours was to be a paraphrase, was itself a celebrated Text-Book. But I still considered it necessary that the work should be well edited for the English market. I could have had it translated verbatim for £50, and should have acquired an incontestable copyright of that translation. I preferred to give you £200, because I wished to have the influence of your name as a Professor, and the benefit of the sale that would arise when the book became a University Class-Book. Your claim to the book as your property, because you intend to use it as your Text-Book, stands therefore on nothing. What you undertook to write for me was, one of my series of Manuals, and, as such, I claim the present work as mine, upon payment to you of the £200 agreed upon for the writing of it.

You must permit me to point out to you some of the unreasonable points in your claim, upon which, as it bears upon my interest, I think you cannot have duly reflected; for I am sure I only do you justice when I assume, that you are not knowingly proposing to take from me what is not yours. The expense of preparing and establishing any Class-Book on a scientific subject, is so great, that a first edition scarcely ever repays the outlay. Besides authorship, there is a multitude of charges to defray. In the present example, the engravings cost £100, advertising will cost about as much, besides copies to be given to the public libraries, to reviewers, to professors, and the like. Thus, in your present letter, you ask for twenty guineas' worth of the book to be given away, at my expense, to promote the establishment of the work. Farther, the corrections on the proofs have been very expensive. All these, and similar charges, fall on the first edition of the work. Now, what you ask is, that these preliminary expenses, which, including authorship, will amount on this volume to pretty nearly five hundred pounds, shall fall to my share, but that I shall have none of the resulting profits. There will be no profit on the first edition. You propose that I should defray every expense attending the production and establishment of the work, including your copyright money of £200; and then, if the work *fails*, I am to suffer all the loss, but, if it *succeeds*, you are to take the successful book, with power to resell it to anybody else, leaving me nothing. In this you do not merely ask to get back the product of your own labour, after having been paid for it, but you ask for the product of my labour and expenditure in that and every other direction. In asking for this, you ask for what is not yours, nor ever was, but what will arise from payments made by me to many different persons, to yourself as one among many. Of course, I cannot agree to your proposal, which is at total variance with my bargain with you.

I have already told you that I am ready to enter into any fair and reasonable arrangement which shall secure to you a proper control over the contents of future editions of the work, and that I am willing to pay you a fair sum for your labour in revising such editions. If you desire any thing else that I can agree to in reason, I shall be glad to meet your wishes. Whatever you may have to propose, I shall consider in, I trust, a fair and liberal spirit, but I will not sacrifice the property as you propose.

In a letter from the printer, dated 19th April, I learn that the proofs were not then all out of your hands.

Your wishes to have the work ready by the 1st May shall be complied with as far as it rests with me.

I remain, DEAR SIR,

Yours respectfully,

JOHN J. GRIFFIN.

No. 13.

To J. J. GRIFFIN, ESQ.

EDINBURGH, 20th June, 1850.

SIR,

In reply to your letter, I beg to state that I have resolved to have nothing more to do with the Manual of Botany. I am making preparations for an elementary work on Botany, over which I am to have complete control. I shall never give my name to any work of which I do not retain the copyright. I certainly would never have written the Manual had I not supposed that such was the case in regard to it.

I am, SIR,

Your obedient Servant,

J. H. BALFOUR.

No. 14.

To PROFESSOR BALFOUR.

LONDON, 27th June, 1850.

DEAR SIR,

Absence from town prevented my replying earlier to your favour of 20th June. I write now to urge you to reconsider a resolution which you have made in a moment of pique, and which I think you will abandon when you reflect on the consequences that must flow from its adoption. I have already offered you the complete control, Literary and Scientific, of your Manual. You can, whenever it is reprinted, make it what you wish it to be. What more do you desire? Is it money? I offered you a sum for the correcting. I said £50. You demurred. I ask what sum you seek? You reply, that you will have nothing more to do with it, that you are preparing a new work—an opposition work. As I wish to induce you to reconsider this matter, I must recall to your recollection the fact, that when you undertook to write the Manual, you pledged yourself decidedly, I think sincerely, to use the work as your Text-Book. That pledge was repeated a dozen times in your letters. In one you say,—

“I told you that I would make the work my own Text-Book, and that I would arrange it in the way I thought best for the purpose of teaching.”

Again,—

“I also think it right to assure you, that it is my full intention honestly to promote the sale of the book, not only by using it myself, but by endeavouring to get my friends to recommend it.”

You will indeed not deny that such a pledge was given. What am I to expect from it? Will you fulfil it or break it? Have you determined to write another Manual, and use it as your Text-Book? If so, what you intimate to me is, practically, that you will, as far as it lies in your power, destroy the property that I have purchased and paid for, and that you have formally assigned to me. Will that procedure be *just*? or can I be expected to admit that it is consonant with the terms of our bargain, of which the pledge to use the Manual as your Class-Book was as specific a portion as the pledge to write it. You must excuse my putting the case before you thus clearly. It is a matter of importance to both of us, and still capable of amicable arrangement. I hope, therefore, you will be induced to take more than a one-sided view of it, and to abandon a resolution which, I am satisfied, you cannot entertain conscientiously. I need not trouble you with a review of the consequences that must result from a persistence in your present resolution, because the impossibility of “*repudiating*” an existing recognised publication, with other awkward particulars, will no doubt as readily occur to you as to me.

Appealing, then, from Professor Balfour in a state of anger, to Professor Balfour in a state of calm reflection,

I remain, DEAR SIR, yours truly,

JOHN J. GRIFFIN.

No. 15.

TO THE EDITOR OF THE “NORTH BRITISH AGRICULTURIST,” *in reply to certain unfounded malicious falsehoods contained in a Review of the Second Edition of PROFESSOR BALFOUR’S MANUAL OF BOTANY, just published.*

—————, May 12, 1851.

SIR,

Your paper of the 7th instant has just been transmitted to me by Messrs. Griffin & Co., publishers of the Second Edition of Professor Balfour’s Manual

of Botany; and as the said paper contains an abusive review both of the work and of its editor, I expect that you will allow me, the editor in question, to lay before the readers of your journal the following justification of the Manual, as a refutation of the aspersions cast on my literary and scientific character.

The first charge alleged against this edition of the Manual is as follows:—"When the first edition of the Manual was published, (as a class-book, not with the view of forming the article Botany in the Encyclopædia Metropolitana,) we thought it a good introduction, &c., and recommended it as a suitable guide to all students of Botany." In reference to this curious complaint, I would ask—and every reader of this defence may well ask—is the book in question the worse for forming part of a series bearing so creditable a title as that which the Encyclopædia Metropolitana has deservedly acquired? It cannot be said that it is worsened by appearing in bad company. Is its reputation tarnished, or is its suitability as a class-book blemished? It is now connected with a work of established reputation, a work produced by authors of the very highest eminence in their respective departments both of literature and science. "Had this volume," (the Manual,) the reviewer continues, "been merely a correct reprint of the former Manual with all its minor faults, &c., it could still have been pointed to as an able and comprehensive exposition of the science of Botany, up to within a short period of its publication." This is an unqualified assertion that the Second Edition is *not* a correct reprint of the work. The copy, however, which I corrected for the press, and which I have now before me, will prove that the present is several hundredfold more correct than the first edition. The father of lies might have been the author of this accusation against me and my labours in correcting the work. The book itself plainly contradicts the calumnious assertion; even the reviewer himself, if he will take the trouble to compare the second with the first edition, must be convinced that the two editions are not only essentially, but verbatim, the same, and that the first has been reproduced in the second edition minus some hundreds of errors which have been corrected. Had I been the author or compiler of the work, or could I have taken the liberty of dealing with it according to my own views of what an elementary work of science should be, I would, *pace tanti nominis*, have considerably altered and amended the Second Edition; my corrections would not have been confined to errors in orthography, etymology, and syntax, but would have extended to the technicality, the phraseology, and general style of the work. Delicacy to the author, respect for the established usages and courtesies prevalent among literary and scientific authors, and the *express and positive letter of instructions from the publishers*, are ample reasons for my leaving the Manual as I found it, minus the above mentioned notorious blunders.

"The professor" is reported to have "*told his class that the edition has not been edited nor corrected by him; that he is not responsible for it; that in fact he has not sanctioned its publication.*" I will not animadvert on the amusing curiosity of both language and logic of this declaration of the professor's, but I will make a remark or two on its morality—I mean the doctor's. It is true that the professor did not edit the second edition of his own book, and no doubt he had satisfactory reasons for declining this office. I do not of course blame him for this nor for publishing it to his class. But I have reason to blame him for making me responsible for the "many imperfections" which the reviewer urges *now*, be it understood, against the Manual, for laying every one of his own culpabilities, whatever they may be, to my charge. If the work was faithfully edited, as I maintain that it was, for an editor's duty is neither to add nor to retrench, but simply to reproduce the original as perfectly as possible, it cannot be said to be worse than it was originally. If Dr. Balfour is not held responsible for every fact, argument, and expression contained in his Manual, I am irresponsible for the contents of this letter.

My editorial intromissions, which were the occasion of the unpolite notice in your paper, extended no further than merely reading the work previously to its being printed; to altering the orthography where wrong, and to correcting the solecisms. I only rectified the professor's or the printer's blunders, and I assert that Professor Balfour ought to feel grateful for such services performed in behalf of his authorial reputation. I urge the unfairness of his making me the scape goat to bear the blame of his own admitted delinquencies.

When the reviewer states, "that there is nothing new in this edition deserving of the slightest commendation," I at once cheerfully admit his statement. I have no right whatever to any praise for the introduction of novelties; for this sufficient reason, no novelty is introduced, the original is exactly reproduced in the present edition, *minus*, as already stated, *a multitude of errors* very disreputable to the persons who superintended its former publication. I, however, scornfully and indignantly reject the following:—"In all instances of alteration and attempted emendation, the severest censure is due for the utter want of judgment and know-

ledge of the science." The falsehood of the above quotation is matched by its malignity. A serious refutation of a pure fabrication, after what I have already shown to be the facts of the case, is utterly needless. My indignation at this gross and unwarrantable attack on my character may possibly be considered somewhat uncourtous, but the provocation justifies the strong language of the expression. I reassert that the accusation is groundless, and I hereby challenge the defamer to specify the facts, and adduce the reasons on which he founds the above allegations in reference to my acquirements and capabilities. I dare him and defy him. Let the corrections which I have introduced be fairly contrasted and confronted with the words as they originally stood in the first edition, and then let the public decide which of us is wrong, and which is utterly destitute of judgment and scientific and literary knowledge. The *ipse dixit* of an anonymous slanderer is not to be judged more worthy of credit than the decided assertion of an editor who refers to the work itself as an incontestable memorial and confirmation of the justice and integrity of his editorship. A very brief comparison of the two editions will be enough to convince every candid inquirer that the work is verbatim the same as the first edition.

It is unnecessary to restate all the mendacious assertions of the review, *ex uno disce omnes*. But it may not be unprofitable to advert to this characteristic of the article, viz., that from its commencement to its conclusion there is not so much as one inaccuracy or error distinctly specified. How do you account for this Mr. Editor? Is it the principle on which your Journal is conducted to condemn a book unseen, or to blast the reputation of an innocent, unconscious rival, without a specification of the faults laid to his charge? Is the office and dignity of the reviewer promoted or magnified by the circulation of vague charges, *Spargere voces in vulgo ambiguas et quærere conscius arma?*

The reviewer proceeds, "In fossil Botany the recent important researches of Brongniart are not even alluded to." Possibly not; but what of this? An Introductory Manual of Botany is not a Fossil Flora. There is already by far too great a conglomeration of subjects in Professor Balfour's Manual. In a geological work, an enumeration of recently discovered fossil plants, with their description, would indeed be both useful and desirable, but few would consult a botanical work for any such information. The scientific treatment of inorganic matter and of organic remains belongs to the chemist and the geologist, but is universally admitted to be beyond the province of the botanist. Again, "The number of fossil plants is stated far below what Unger has described." The professor himself has stated the number of living cryptogamous British plants far below what Hooker, Greville, Berkley, Hassel, Ralf, and Jenner have described. But I do not consider this as a serious blemish to his reputation as a botanist, I only cite it as an authority to which my captious critic may defer. The most comprehensive works on species are necessarily incomplete in this respect. The progress of discovery has been recently very much accelerated, while the composition or compilation of books is still a tedious manipulatory process.

Again, "In botanical geography the erroneous statements pointed out by Hooker and Watson have been left unaltered." What statements? Where and when were they pointed out? "None of the valuable remarks in Mr. Watson's *Cybele Britannica* have been attended to." The *Cybele* is not yet finished, and if it were, it will not affect the concise account derived from Mr. Watson's earlier works on the geography of British plants. But it is very unreasonable to expect that a work like the Manual should exhibit a full view of Mr. Watson's valuable labours on this subject, contained as they are, or will be, in five or six volumes. Another complaint is, that no improvement has been made in the arrangement and description of the natural orders, and in the account of their properties. The botanical orders have been too often treated like shuttle-cocks, botanists have too frequently displayed their ingenuity in proposing arrangements differing from those of other botanists, and, no doubt with sufficient cause, have occasionally differed from themselves. As no two botanists can or are ever likely to agree in the arrangement of the natural orders, it might be as well for science to let them stand where they are, and study them rather than squabble about their classification. The properties of individual plants, and the characters of the orders in which they stand are unalterable, except through culture, and this is the business of the forester, the agriculturist, horticulturist, and the florist. No botanist has ever been expected to teach all these distinct independent sciences. The natural and essential characters of every individual of the whole vegetable kingdom are and ever must be the same as they were at the beginning of their creation. The hog-weed is just what it ever has been; the carrot has been changed by cultivation, though still found in its natural state. Both are objects of botanical study, but the latter only in its primitive condition. Neither the publishers nor myself have

the slightest wish to get praise where none is merited; but unfortunately we can neither please our censorious critic by leaving things as they were, nor alleviate his resentment by altering them. It may be inferred from the exordium of his critique, that the former did amiss in publishing the second edition as a portion of the "Encyclopædia Metropolitana," and I am blamed expressly for not altering the description of the characters and properties of the natural orders—a part of the subject which, if rightly handled in the first edition, has certainly undergone no change, nor ever can. *Nescio quo teneam Prete!* We have not altered the unalterable characters and properties of plants; this is bad; but what is worse, "glaring errors have been introduced" into a subject where amendment was not, as in the former case, impossible, namely, into the orthography of Hebrew names. The reviewer, who, if I may judge by what follows, knows as much of Hebrew as he does of the principles and practice of honour and veracity, states, "the Hebrew names are in many cases quite wrong, the editor not being able to distinguish between the letters *He* and *Cheth*," not *Heth*, as he writes it. I will make him a present of the following information, it may be of some avail to him when he begins the study of the sacred language. *Cheth* is the Hebrew representative of the Greek Chi (χ). If he knows no more of the latter language than of the former, I tell him that ח or χ has the guttural sound of *ch* in the Scottish word *clachan*. He may understand this. "The editor being unable to distinguish between the Hebrew letters, *He* and *Cheth*, not *Heth*, *Vau*, *Kesh*, and *Daleth*, *Nun* and *Gimel*, &c. To the student of Scripture plants the work has become a false guide." From the above it might justly be inferred that the Manual abounded in Hebrew names of plants, and that I had altered such names as were partly composed of these above-mentioned letters. The fact is, however, of another kind, and will speak for itself and for me too.

The Hebrew terms representative of Scripture plants contained in the Manual amount to 29, of which 13 are wrong in the first edition. I corrected the following:—

In paragraph 764, I changed אָוֹב into אוֹב

In paragraph 891, I changed חִלְבָּנָה into חִלְבֵּנָה

In paragraphs 967, 994, 1003, and 1037, I changed final aleph א into initial aleph א

And in 907, נֶרֶךְ into נֶרֶךְ

All changes as necessary as they were obvious. This curious connection between ignorance of Hebrew, and the conceit and pedantry of entering Hebrew terms in an elementary work, without so much as deigning to present equivalents in the English characters, and even without the Hebrew vowel points, I submit to the explanation of the author. But why cannot the few Scripture plants described in the Manual be studied without the learned parade of Hebrew names? Hyssop is the same whether it be understood by the terms *Hyssopus*, ὕσσωπος, or יָאוֹב, and affords the same characters and properties for the student's investigation. The Hebrew terms may be right or they may be wrong without affecting the suitability of the Manual as a guide to the student of Scripture plants. As I have sufficiently cleared the Manual from the imputation of being an unsafe guide to the knowledge of plants mentioned in the Bible, my own justification rests solely upon the corrections made by me, or rather upon the non-corrections, for I am accused of making corrections which I did not make. I altered

אָוֹב into אוֹב on the authority of the Hebrew Bible, Lev. xiv. 4.

חִלְבָּנָה into חִלְבֵּנָה — — Ex. xxx. 34.

נֶרֶךְ into נֶרֶךְ — — Canticles i. 12.

And in reference to the terminal aleph א which I changed into the initial א, see Hebrew Bible, *passim*.

The errors in the Hebrew names of plants are not of my introduction, and however many there may still remain in the Manual I am not responsible for them. On this point I will willingly submit to the decision of any competent Hebraist, if there be any such who can convict me of utter ignorance of the language, from my alterations as above-mentioned. It may be true that the Hebrew names are in many cases quite wrong. Why did not the author correct them? And why does the critic charge me with glaring blunders in the matter of Scripture names of plants, when he might have seen, if he had taken the pains to look into the Hebrew Bible, that the few corrections were made necessarily? But I fear I am doing the critic wrong, and that he knows no more of Hebrew than he does of a good conscience. It might possibly be

supposed that he thought "the Hebrew names in many places quite wrong," but a different conclusion, legitimately deduced from the spirit and matter of his review, might be adopted, without any breach of charity, viz., that he wished the fact to be as he states it, and that the wish was father to the thought.

The critic has still another and a graver charge against me, supported by what he may possibly think facetious banter, but which, in fact, is as destitute of the *curiosa felicitas* of your ingenious contemporary *Punch*, as the statements of his stupid critique are devoid of all truth. "The natural order Rhizanthæ," he continues, "may be noted as an extraordinary case fixed upon by the editor, as a convenient one for exhibiting new views of classification, and he settles all disputes," of which he admits the existence, "by wedging it into the sub-class glumaceæ." "Verily this is worthy of *Punch*," &c.

It might be inferred from this senseless remark of the critic, firstly, that this convenient case is only a solitary example of several others, wherein I have indulged my classifying propensities; and, secondly, that I had introduced the order in question in the midst of the glumaceous orders. It is said to be "wedged in," and a wedge is never inserted but somewhere *between* the two sides of a thing which is to be split by its means. An ounce of fact is better than a stone of assertion. In the first place, the order is not introduced among the glumaceous orders, but follows them as it does in all the arrangements of the natural orders that have recently been published. Endlicher's Meletemata, p. 10, is quoted by Lindley in support of his views of the affinities of this curious order. The former, viz. Endlicher's work, was published at Vienna, in 1832, and Lindley's Natural System at London, in 1836. The article, as condensed or abridged by the latter botanist, is by far too extensive to be quoted here, but it is to be seen in the last quoted work, second edition, Lond. 1836, pp. 389, 393. The Vegetable Kingdom comprehends the most recent modifications of Dr. Lindley. But the class Rhizogens, equivalent to Balfour's natural order Rhizanthæ, still occupies the same position in the system, exactly where I placed it. See Lindley's Vegetable Kingdom, page 4, where the class Rhizogens is placed between Acrogens and Endogens; and page 83, where the Rhizanthæ are fully described. See also Balfour's Manual, page 351, which exhibits Endlicher's views of classification in 1836; and where Hysterophyta, equivalent to Rhizogens of Lindley and Rhizanthæ of Balfour, is inserted between Acrobrya and Amphibrya. The sole authority for its position in the first edition of the Manual, and which the author did not quote, though it is to be found only in a series of publications which hundreds of his readers may never see, has remained silent on the subject for many years; and it may reasonably be inferred, that as the original authority for the Doctor's classification has offered no opposition to the more recently advanced views, that his opinion has undergone considerable modification on this point. I am sure every botanist who has the least botanical knowledge of the habit, structure, and fructification of this anomalous order, and who is not deaf to the word of authority, will admit that its place in a systematic arrangement is nearer Acotyledonous than to Dicotyledonous plants, among which the author of the Manual placed it.

Another word on the assertion that "it is wedged into the subclass glumaceæ," or, which is the same thing, that it is introduced among the glumaceous orders. It is not so placed, it is placed *after* the glumaceous orders. It would have been far more convenient to me to have made a class of Rhizanthæ, after Lindley's example, and to have changed the number of the class Acotyledones or Acrogenæ into a higher number, viz., from Class III. to Class IV. But I consider that this would have marred the harmony of my author's system, by introducing Rhizogens or Rhizanthæ, names which are inconsistent with his terminology; and to have adopted Lindley's arrangement and nomenclature would have essentially changed the character of his work, which the publishers instructed me *not to do*. That it is in its right place there is now no doubt, nor has there been any dispute about it for the last twenty or thirty years. The cavil, then, can only be directed against its standing next to Gramineæ. Now as Gramineæ is the last order in Balfour's arrangement, it must necessarily be set there, and before the III. Class Acotyledons. Would the reviewer be satisfied if a blank space of the page had intervened between Gramineæ and Rhizanthæ? Even if the word class and subclass, which after all are only abstractions, and have no reality in nature, had been placed before the Rhizanthæ as a heading, they, the plants of said order, would have occupied the same proximate station to the grasses, as being the last order of strictly phanerogamous plants, as they now occupy in the Manual, and which they occupy in Lindley's system, viz., after *glumosæ*, and in Endlicher's system as the highest order of Acrobrya. When Professor Balfour next publishes his views of classification, I shall be curious to see what place the order Rhizanthæ is doomed to occupy in his system. He will scarcely have the hardihood to follow an opinion long exploded, viz., that it has any remark-

able affinity to Aristolochiaceæ, and that it belongs to the orders of Monochlamydeous dicotyledons: its organs of fructification (stamens and pistils) separate it from Cryptogameæ. He must then leave it where it is, or place it where I have placed it, between Monocotyledons and Acotyledons, or reject it altogether.

The order in question was removed from its former to its present place in the Manual after due consideration, and it is unfair to fix on me the charge of placing it among the Glumaceous orders, when it is only in juxtaposition with them. The insertion of a classical designation would not have affected their position: such a designation is only an arbitrary conventionality, useful in practice, but which has no reality. I considered that its former situation was a mere oversight of the author's, similar to the placing of *Oxalis* nat. ord. *Oxalideæ* in the order *Polygonaceæ*, misled, no doubt, by its English name Wood-sorrel, and which, bearing some similarity in its name to common sorrel, was left by him in an order from which it is naturally far apart.

The slight change introduced by me, authorized as it is by the most eminent systematists of the present or any other age, is not a satire of any kind or degree upon "all systems of classification and a perfect outrage upon common sense;" but the critic's audacious, impudent, and false assertion is so, and on common honesty to boot. Professor Balfour is of course justified in taking any steps he pleases to put the public in possession of the fact or information that he is not responsible for this slight change, I also, Mr. Editor, will take sufficient care that my honour as a gentleman and my professional character as a professor of Botany shall not be blemished with impunity.

The cavils about the old tables of chemical analysis form a remarkable contrast to the above. I am blamed for introducing a novelty in classification which has been known to the British public above 15 years: and, again for retaining old tables of chemical analysis, which, if the results of accurate chemical analyses, will be the same for ever. It signifies little to the student whether they be new or old provided only that they be correct. The professor, I beg his pardon, the reviewer is mum on this essential requisite and character of the tables. The twaddle about cell-development is both childish and impotent. All this was well known long before the first edition of the Manual was published, and it is reasonably to be inferred that the author introduced from Schleiden's work as well as from other recent publications, on the structure and physiology of plants, all that suited his purpose or all that were requisite for the elucidation of his own views of the whole subject. To infer that he acted otherwise would be tantamount to the charge of imposing on the publishers and the public both. But why, we may again ask, does not our anonymous accuser tell us what those defects are which might have been removed by the able developments of Schleiden? What are the so called recent views of embryology? Lack-a-day! they are multifarious and multiform like classified arrangements of the natural orders. Very few botanists are agreed about the *modus operandi* of fecundation or fertilization. Does our critic know how to explain the mystery of generation in general? Recent views of embryology indeed! and agricultural investigations of Anderson and Johnston; Dr. Wilson's discovery of fluorine in plants, and Mr. Way's discoveries of the absorbency of clay. All very excellent, and in their proper place, very desirable. *Sed hic non erat locus.*

The unconscionable and unreasonable critic would have an encyclopædia, a complete circle of scientific knowledge, chemistry of organized and inorganized bodies, meteorology, agrarial and horticultural science, geology, a complete enumeration of all plants that have existed, do, or ever may exist, all within the compass of a manual, a hand-book, a pocket volume, and all for the small price of a few shillings. Enough has been already laid before the reader to evince the *animus* of this pseudo-critique; enough to convince the unprejudiced and disinterested lovers of science and of plain honest dealing, that the reviewer, unworthy of the honourable profession, is actuated solely by the foulest and ugliest motives.

But there still remains another charge, to which I only allude *en passant*, in order to dismiss it with the contempt which it deserves; I mean his paragraph about the microscopes. The account of these instruments was inserted by the express desire of Mr. John J. Griffin, and to him I leave the task of their vindication. I need only remark, that it is nothing more nor less than a glaring absurdity, to recommend students of botany to purchase a microscopic apparatus, which would cost from £60 to upwards of £100, when he must know how few botanists or physiologists there are who can afford to invest so much money in a microscope.

The serious regret with which he pretends to dismiss the Manual with the following condemnation, is only calculated to deceive such as look through the bleared and jaundiced optics of the reviewer. "The work might have been a valuable one in qualified hands, but has been spoiled and rendered ridiculous by bad editing."

He knows, or he ought to have known, before he summarily and superciliously condemned both me and my work, that the Manual is *bona fide*, essentially and literally just the same as the first edition, previously praised by him, but minus the mistakes of that edition. It is not, nor does it profess to be, an edition with additions and emendations, it is a mere reprint of the original, deducting the errors, which a careful perusal and revisal of the book and of the press discovered and cancelled. If there are any blunders in the work, they have *not* been "brought together by other hands," as it is wickedly and foolishly affirmed; but they are such as were originally inherent in it, and of which, except by vague indefinite accusation, our critic does not indicate so much as *one*. If the Professor's well-merited reputation is in jeopardy the fault is his own. I made no additions, amendments, nor alterations whatever, but such as were necessary to render the work as correct and as consistent with itself as possible. The distortion complained of, consequently, cannot be in this portion of the Professor's writings, but rather in the moral optics of the individual who has looked with an evil eye on my unpleasant task of rectifying so many blunders and inconsistencies. I should indeed have reason to sympathize with the votaries of science in general, and with the promoters of Botany in particular, for with many of the latter I have long been intimately and agreeably connected, if the Professor of Botany in one of Scotland's most famed universities had the least connection with the stupid farrago of blunders and abuse which the reviewer has published in your journal.

I am, Sir,

Your obedient Servant,

THE EDITOR.

No. 16.

Specimens of the 409 Errors CONTAINED IN THE FIRST EDITION of Professor Balfour's Manual of Botany, and CORRECTED IN THE SECOND EDITION.

Bad English.—P. 52, The *composition* of the vascular bundles, in different parts of their course, *vary*. P. 110, The *tissues* into the composition of which these proteine compounds enter, *is* tinged, &c. P. 144, Beet-root and white turnips *contains* only 3 per cent. P. 196, A *Filiform* filament.

Mistakes in Proper names.—Meyer for Meyen; Göethe, and sometimes Goëthe, for Goethe; Micham for Mitcham.

Mistakes in Latin.—P. 12, *fissum* for fissus; in p. 74, *fidus* for fissus. In above a dozen places, Latin verbs are quoted in the first person of the indicative mood, and the English translation is given in the infinitive, making, for example, *to bear* to be equivalent to *I bear*.

Mistakes in Greek.—As in the Latin, so in the Greek, the English is put in the infinitive, and the Greek in the indicative, mood. There are above 30 errors of this sort. At p. 163, we are told that *ἐλκῆ* signifies a *ring*, and at p. 594, that *ῥῶς* signifies *drawn*. They give these words other meanings at Oxford and Cambridge, and those meanings have been put into the Second Edition. These, perhaps, are the *changes* which struck the reviewer as being "of a sufficiently odd and remarkable character."

Errors in Matters of Fact.—P. 354, Henslow gives the following analysis of these sub-orders, with the number of British *genera* in each—[read *species*.] P. 269, "While the fruit enlarges, the sap is drawn towards it, and a great exhaustion of *ITS* juices takes place." [Of course, by the time the fruit is ripe, it will be *totally* exhausted of *its* juices.] P. 149, *Reseda Luteola* is interpreted to signify *Woad*, and is classed among plants that yield *yellow* colouring matters. I take leave to state that *Woad* is a *blue* dye, and that it is derived from the *Isatis tinctoria* of Linnæus, a plant which, under that name at least, is not to be found in the Manual. It is not even named among the plants that yield blue dyes. On the contrary, the *yellow* colouring matter derived from *Reseda Luteola*, is not *Woad* but *Weld*.

Professor Balfour is said to admit his responsibility for the First Edition of his Manual, but not for the Second Edition. As the latter differs from the former merely by the absence of these 409 errors, perhaps he would like to see them printed as an Appendix to the Second Edition, to COMPLETE IT, and restore his responsibility.

S U P P L E M E N T.

A LETTER TO

R. CHRISTISON, M.D., V.P.R.S.E., Professor of Materia Medica and Clin. Medicine.

JAMES SYME, Esq., F.R.S.E., Professor of Clinical Surgery.

JAMES Y. SIMPSON, M.D., F.R.S.E., Professor of Midwifery.

JOHN GOODSIR, Esq., F.R.S.S. L. and E., Professor of Anatomy.

J. H. BENNETT, M.D., F.R.S.E., Professor of Institutes of Medicine and Clin. Medicine.

DOUGLAS MACLAGAN, M.D., F.R.S.E., Lecturer on Materia Medica.

WILLIAM ROBERTSON, M.D., Physician to the Royal Infirmary of Edinburgh.

LONDON, *June 10*, 1851.

GENTLEMEN,

After the preceding letter to Professor Balfour was in type, I received the "*Monthly Journal of Medical Science for June, 1851.*" On the title page of that work it is stated that you are its CONDUCTORS. At page 541, I find in it an article in the form of a Review of Professor Balfour's Manual of Botany, but a Review in which the ordinary license of criticism is exceeded in so extravagant a degree, that I take the liberty to protest against it publicly. The Review is no example of the sound criticism that one is entitled to expect from Edinburgh Reviewers. It is a Sham Review—an imposture.

The following is the article:—

REVIEW.

Manual of Botany, &c. By JOHN HUTTON BALFOUR, M.D. Second edition. Griffin & Co., London. 1851.

Encyclopædia Metropolitana, &c.; Second Division—Applied Sciences—Botany. Griffin & Co., London. 1851.

1. THESE two titles belong to one and the same book, a second edition of Dr. Balfour's well-known and deservedly popular Manual being made to serve as the volume on Botany in the Encyclopædia. To such an arrangement there can be no objection, provided that this so-called second edition is such in reality, and that it has been duly corrected and brought up to the actual state of our knowledge. But such is not the case in the present instance. This may be called a second edition; but it is in fact little else than a reprint, with this remarkable peculiarity, that *most of the alterations made in it have only served to introduce errors.* It almost seems as if this reprint had developed itself by a kind of spontaneous generation. It has lots of title-pages, and bears the preface of the first edition, with the date omitted; but it has no preface of its own to explain in what respect it differs from the first, and might be a thing of spontaneous, we may certainly say of equivocal, generation, but that at page 613, a mysterious personage, subscribing himself "Editor," makes his appearance in connection with a parenthetical sentence, containing an advertisement of certain microscopes manufactured and sold by the publishers of the volume.

2. *We leave it to those journals whose province it is more particularly to attend to the interests of natural science, to expose, if they think fit, the errors which this book contains.* All that we purpose to do is to protest, as an Edinburgh journal, against the supposition that this book indicates either the actual state of botany among us, or the way in which science is cultivated here. We hope it will not be thought that our ideas of a second edition are, that it is good enough, though it may not contain a syllable regarding all that has been done for two years back. *It may be all very well for the subscribers to the Encyclopædia Metropolitana to be supplied with such musty information,* and if they are content, we, not being of their number, have no objections to offer,—*chacun à son gout*; we like our information on science to be fresh, even though it

should make the reading a little tough; theirs is well kept, and ought to be tender, and so we wish them a good digestion of it.

3. It behoves us, however, to explain to our readers that in making these remarks we have no blame to lay at the door of the Professor whose name the book bears. *He has himself, publicly in his class-room, and also at a meeting of the Botanical Society, where he presided, disclaimed all connection with, and responsibility for, this edition of his Manual*; he is content to bear the onus of whatever errors the original edition contained, but he is not chargeable with the faults, either of omission or commission, which pertain to the second; and in justice to him we must shortly explain how this comes to be.

4. When Dr. Balfour published his first edition, he was, though an old botanist, *only a young author*; however well acquainted with the scientific arrangements of the vegetable kingdom, he knew but little of *the mysterious commercial arrangements* of the republic of letters which refer to the sale of copyright. With respect to these, he was in what we shall, since writing on a botanical subject, venture to call a state of ehlorophylle—in short, “green”—and *he speedily found that, when he had sold his first edition to his publishers, the terms which he had made were such that he had unwittingly parted with the entire copyright of his work*. When a second edition was called for, by the rapid sale of the first, he found himself in the predicament of being called upon to superintend its preparation *on terms with which he could not comply*. Under these circumstances, and acting under the advice of his friends, he had no option but to abandon his Manual altogether, and leave it to those who had in this way acquired it, to make of it what they liked.

5. *What they have made of it* may be gathered from our opening sentences. Being unable to compel Dr. Balfour himself to edit it, they have employed somebody to do the work in his place; and the qualification of this unknown “Editor” to superintend a botanical publication may be judged of by one instance, which is too startling not to have attracted, as it has already done, the notice of botanical reviewers elsewhere.

6. There is a remarkable set of parasitical plants called Rhizanthææ, which agree in many points of their general structure with fungi, but which are, in respect of their reproductive organs, closely allied with endogens. They have been a sad puzzle to systematic botanists; and in order to find a resting-place for them, Lindley has made of them a separate class, Rhizogens, between Thallogens and Endogens, in which, however, he has not been followed by some other botanists of authority, as Robert Brown and Griffiths. The “Editor” of the present book, apparently attracted by a sentence in the original edition mentioning this arrangement of Lindley, but totally neglecting all the rest that was said regarding them, seems to have been inspired with a hidden wish to do something remarkable with them, and he certainly has succeeded to his heart’s content. They were to be placed, according to Lindley, as a class, between Endogens and Thallogens, and there he certainly does put them, not as a class, however, but as an order, and under the last tribe of the Endogens, the Glumaceæ,—the Sedges, and Grasses. In short, he does not know the difference between a class and an order, and puts “parasitical plants, destitute of true leaves,” whose “stem is either an amorphous fungous mass, or a ramified mycelium,” which are in “no instance of green colour,” and the “true nature of whose seeds is in most species quite unknown,” with the long-stemmed, long-leaved, fresh-green, large-seeded Carices and Grasses. We do not know whether the “Editor” has ever tried his hand on any thing zoological, but if he has or does, we need not, after this, be surprised if we should find in his classification the cameleopard keeping company with a generation of tortoises, or the hippopotamus agreeably disporting himself with a family of lobsters.

7. *Of omissions the catalogue is endless; little that originally was or has become erroneous is corrected, and every thing new is omitted in every department*. To take one example, which is of interest to us in a medical point of view, the yellow Cinchona still stands in the list of barks as being furnished by an unknown tree, whilst nearly about the time that the first edition of Balfour’s Manual was published, Weddel’s researches were given to the world, by which it was made known that this valuable drug was yielded by a new species of Cinchona, which that gentleman named C. Calisaya. The carelessness with which even the mere mechanical work of superintending the reprint has been done is conspicuous. Thus in noticing the Narthex Assafoetida, which is growing in the Edinburgh Botanic Garden, the plant is stated to be two years old. That was true in 1848, when the earlier part of this Manual was published. It of course is not so now; so that this interesting plant stands still in point of years, however progressive in point of growth, somewhat like a lady of a certain age of whom we have heard, who has returned herself as 30 in three successive decennial censuses.

8. But we need not pursue this farther. Enough that we protest that this edition

is not to be taken as a sample of Edinburgh scientific literature, being, as it is, neither as a Manual nor as an encyclopædic treatise, a correct representation of the present state of botanical science.

An article similar to the above appeared, on the 7th of May, in the "*North British Agriculturist*." It is quoted in the preceding Appendix, No. 1. It was published anonymously, but I have been told that it was written by one of you. The articles bear, indeed, internal evidence of being the work of the same mind. There is in both of them the same mis-statement of facts, the same course of paltry insinuations, expressed in the same slip-slop language. Like the Parasite of Antiphanēs, the writer may take for his motto—

"I'm for all work, and though the job were stabbing,
Betraying, FALSE-ACCUSING, only say—
Do this,—and it is done."

In so far as your article is a Review of the Manual of Botany, it is sufficiently refuted in my reply to the former Review. But the circumstances under which this second attack is published, and the fresh calumnies it contains, demand a few additional remarks. I have shown that the statements made in the "*North British Agriculturist*," and repeated by you, after an interval of three weeks, in respect to *the introduction of errors into the Manual*, are totally unfounded, and that the errors you speak of exposing do not exist. In this pamphlet I have disproved your assertions and insinuations by evidence.

Your declaration, that the *catalogue of omissions is endless*, must have this reply: In the month of April, in the year 1849, your colleague, Professor Balfour, delivered the work to me *as being perfect at that time*. In the month of November, in the year 1850, the second edition was put in hand. When you affirm that *endless discoveries in Botany*, which ought to have been put into the Manual, *have been made since April, 1849*, you betray the trust reposed in you as critics, by trying to trick the public into the belief of what is not true. The *endless discoveries* that have been omitted from the Manual, like the *endless errors* that have been introduced into it, exist nowhere. You cannot prove what you affirm. A brief record of the botanical discoveries made between April, 1849, and November, 1850, which, with difficulty, might be made to fill half-a-dozen pages, is all that could possibly have been added to the work, and that addition would have been dearly purchased at £100; but having made the offer of that sum to Dr. Balfour, I did my duty to the utmost. As you truly say, I was "*unable to compel Dr. Balfour to edit the work*." He is, consequently, alone to blame, if the work is published without additions that might have added to his reputation.

I turn from your review of Balfour's Botany to your notices of myself, and of the Encyclopædia Metropolitana. When an anonymous slanderer is permitted to use the power of the periodical press to serve his personal malignity, and seven Professors of the University of Edinburgh aid and abet him,—it is time for the object of such attacks to defend himself.

In the fourth paragraph of your Review you are pleased to insinuate that I

cheated Dr. Balfour out of the copyright of his *Manual*—that I paid him for one thing and took from him another. You say, that that gentleman, being a *young* author, being *green*, and acting *unwittingly*, was, by some “mysterious commercial arrangements,” deprived of his entire copyright, when he had sold only his first edition. That, consequently, when a second edition was required, “*he found himself in the predicament of being called upon to superintend its preparation ON TERMS WITH WHICH HE COULD NOT COMPLY.*” That, “*under these circumstances, and ACTING UNDER THE ADVICE OF HIS FRIENDS [!], he had no option but to abandon his Manual altogether, and leave it to THOSE WHO HAD IN THIS WAY ACQUIRED IT, to make of it what they liked.*”

There was once a tradition that Authors were helpless Innocents, and that Publishers were Ogres who went about seeking to devour them. But that, in the year 1851, seven grave Professors of the University of Edinburgh—seven Sages of Modern Athens—should collectively affirm such tomfoolery to be matter-of-fact, and that, too, at a moment while boasting of their amazing powers *to digest fresh information, however tough it may be*, is a thing that would be incredible were it not in print before me, prefaced by your names. Fallen, fallen, indeed, is the practice of Edinburgh reviewing!

I should like to know what your colleague, Professor Balfour, says to your agreeable account of this affair. Does he admit that he is, as you represent him, *so young, so green, so unwitting, so incompetent to act for himself, and so submissive to the advice of his friends?*

I should like to learn also *how you knew* that he was in the “predicament of being called upon to superintend a second edition, *on terms with which he could not comply.*” His predicament was, no doubt, very awful. He was in danger of being troubled with a hundred pound note, and with (what he had asked for,) the entire literary control of his *Manual*. Such was the Necromancy which the Ogre exercised against the Innocent. But even if Professor Balfour had told you that he was in the terrible predicament of having to refuse £100 for doing almost nothing, what right had you to pronounce publicly, and upon a private and *ex parte* statement, a decision that impugns my honesty? Is this a specimen of the *law* and *morals* taught in your University? Your business, as reviewers, was with the Second Edition of the *Manual of Botany*, and with that alone. When you insinuate, that by means of a “mysterious commercial arrangement,” I defrauded your colleague of his *entire copyright* when he had sold only his *first edition*, you step out of your true path to do a gross act of injustice, and to propagate an untruth. You profess to know all the particulars of the transaction, and come forward voluntarily to wash Professor Balfour from the stains of blame. If you *do* know the particulars,—those same particulars that Professor Balfour communicated to his legal advisers,—how is it, that those legal advisers admitted that I was in the right, while you declare me to be in the wrong? Your sole argument is, that my commercial arrangements were *mysterious*.—What! Too mysterious for Edinburgh lawyers to see through, and yet transparent to medical professors? The supposition is

ridiculous. If on the contrary, you have presumed to decide as you have done, *without* the information communicated to Professor Balfour's solicitors, then I can only say, that you most truly combine Mr. Wilmott's characteristics of modern criticism—"a want of Modesty and a want of Knowledge."

But you are not contented with defaming the Manual and slandering its publisher, you must also decry the Encyclopædia Metropolitana. You say—

"It may be all very well for the subscribers to the Encyclopædia Metropolitana to be supplied with such musty information, and if they are content, we, not being of their number, have no objections to offer,—chacun à son gout; we like our information on science to be fresh, even though it should make the reading a little tough; theirs is well kept, and ought to be tender, and so we wish them a good digestion of it."

This sentence is a charming specimen of the elegant, scholarlike, honest criticism that can be produced, when seven Edinburgh Professors, the colleagues of Christopher North, combine their wit and wisdom to that end. The humour of the sentence is so exquisite, that I must suppose the mantle of Sydney Smith to have fallen upon you.

If you know any thing of the New Edition of the Encyclopædia, in which Balfour's Botany is now placed, you must know that the insinuation, that it contains only "*musty, well kept, information,*" is unfounded. The publication of such insinuations by men in your positions, can only proceed from an intention to injure me, by damaging the sale of the Encyclopædia. But that intention will be defeated. The Subscribers will deride your puny effort at mischief-making, and will only be amazed that men like you should attempt to overturn such a work, by such a sneer, for such a purpose. The attempt, however, is eminently characteristic of the EDINBURGH PRACTICE OF CRITICISM, which, while boasting in the most conceited phraseology, of the excellency of Edinburgh philosophers, sets veracity and the rights of other men, at defiance. What am I to say to writings which show an evident intention to crush me, and to damage my property? Neither my monetary transactions with Professor Balfour, nor my publication of the Encyclopædia Metropolitana, had any thing to do with the merits or demerits of the second edition of the Manual of Botany, to which it was your duty, as reviewers, to have confined yourselves. You have gone out of your way to slander me, and to depreciate and condemn the Encyclopædia, and in self-defence I am driven to prove to the public by this pamphlet that your published statements are unworthy of credit.

Whatever is "mysterious," as you pretend, in my transactions with Professor Balfour, will be cleared up by the documents that are now presented to the public. My case is very straight forward. I projected a certain publication. I asked Professor Balfour to write a volume. I offered him Two Hundred Pounds for his labour, and he agreed to the bargain—accompanying the agreement with a pledge to use the Book to be written as his CLASS-BOOK. When the work was ready he asked for the money as the price of *an edition* of his work. I refused to pay him, for the reasons I have related, until I had an assignment of the copyright, which he at length gave me. It was an unavoidable necessity. The evidence to prove that I was in the right was too powerful to be resisted. You state that in doing so "he acted under the advice of his friends." I am glad to hear that he has

some discreet friends, and sorry to find that he did not continue to act under their advice ; had he done so, neither your review nor this pamphlet would have had occasion to annoy the public. When the second edition of the work was required, I offered the author £100 to superintend its publication. You stigmatise this offer as being “ *terms with which he could not comply.*” What his expectations were, what your information may have been, whence it came, and upon what ground you make yourselves Assessors, and arrogate the right to decide dogmatically upon a one-sided view of such a matter, I do not know. If Professor Balfour is so young, so green, so unwitting, as you represent him to be, I consider that in paying him £200 for the copyright of a translation of a single volume, and offering him £100 to correct the Second Edition, I made as fair an offer as so young, so green, so unwitting an author had any right to expect. As for *your valuation* of Dr. Balfour’s copyright, the public will perhaps care as little for it as I do. It is enough for you to fix the money-value of your own copyrights, and leave to me the right to value what I have to pay for.

When Professor Balfour refused to edit the Second Edition, I published it without his aid ; and I appeal to all reasonable men who do not belong to the Dicasts of Modern Athens, to judge whether I had not a right to do so, and whether I have not done it fairly. Out of a kind feeling towards the author, I made no substantial alterations in his work ; but I corrected hundreds of errors that disfigured the first edition. He had, consequently, no real cause of complaint. But you have decided otherwise, and the result is a spectacle, in which a body of men, constituting almost the entire Medical Faculty of the University of Edinburgh, are seen at work, using their powers as Editors of public Journals, to undermine my character, and to destroy my property, because, in a case that was purely commercial, I would not agree to what I felt to be an author’s unjust demands—that author being one of your Faculty. I place the facts of the dispute unreservedly before the public, and I feel satisfied that honourable men, who do not belong to selfish Cliques, will do me justice.

It will be a new article in the code of Criticism, if publishers are to be denounced as cheats when they dare to use a property that they have bought and paid for, or when they resist the absurd demands of authors whose vanity leads them to overrate the money-value of their productions. Under such a system, either publishers or reviewers must soon become extinct. No reasonable men will invest capital in publishing if subjected to such coercion as that attempted to be practised on me. Here are seven Professors, in total ignorance of what they write about, declaring, with amazing self-confidence, that when I pay £200 to an author, whom they describe as young, green, and unwitting, it, nevertheless, only entitles me to publish one edition of the work so paid for. The conditions of the contract are disregarded, I am condemned without being heard in self-defence, and punished by lampoons in the form of Sham Reviews.

Professor Balfour’s promises and written pledges, to use the Manual of Botany as his Class-Book, were so specific, that I affirm he has no legal right, as I may have occasion to prove, to publish and use any other Class-Book. It was under the influence of these promises, commencing, as I have shown, in January 1847,

and frequently renewed, that I agreed to pay him a sum of money, which his copyright was not otherwise worth. So far, therefore, from my having “mysteriously” taken from Professor Balfour more than I paid him for, his resolution seems to be, that I shall not receive that for which I bargained and paid. Impartial men will judge of this fact from the evidence I have adduced. Your bold assertions must give place to my documents.

Gentlemen,—I have now given you MY version of this affair, which will enable you to review your own review. The retrospect must convince you that, in quitting the flowery paths of literature to constitute yourselves judges—Dicasts—in commercial matters, you have mistaken your proper functions, and have assumed powers that you cannot be permitted to exercise. Whatever the esoteric doctrines of your Clique may be, the exoteric world, to whose decision you must bow, is still governed by justice and fair dealing. The business of a publisher is not to be willfully destroyed because his opinion of the value of your pet’s writings disagrees with yours. You have taken up a false position. The office of Reviewer does not become you. I recommend you to keep within your College; attend to the duties of your Professorships; and leave Criticism to those who have “knowledge and modesty” to qualify them for its proper exercise.

I am, GENTLEMEN,

Your obedient Servant,

JOHN JOSEPH GRIFFIN.

POSTSCRIPT.

In a few days, Price Twopence,

THE EDINBURGH PRACTICE OF CRITICISM: a Reply to certain SHAM REVIEWS of Professor Balfour’s Manual of Botany. By JOHN JOSEPH GRIFFIN.

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J. J. G.